1903MNRAS..63..281T

ADDRESS.

Delivered by Professor H. H. Turner, on presenting the Gold Medal of the Society to Professor Hermann Struve.

By the award of the Gold Medal to Professor Hermann Struve for his work upon the satellites of *Saturn* the Council have recognised by the highest means in their power the great value and importance of his contributions to our knowledge of the Saturnian system. It will be seen from the account which I now proceed to give of Professor Struve's researches that they combine in a remarkable manner great observational skill and perseverance with difficult and complicated mathematical analysis and numerical reductions. Professor Struve's earlier observations, which were made with the 15-inch refractor at Pulkowa, were published, together with the investigations derived from them, in a supplement to the Pulkowa observations in 1888.[*] The later and more elaborate series of observations were made with the 30-inch refractor at Pulkowa: they were published with a very full discussion in a separate volume of the Pulkowa publications in 1898.[†] These two volumes contain an immense amount of material relating to *Saturn's* system, and also a complete account of the methods by which Professor Struve has himself derived from them so many results of the highest value and interest. During the progress of the work several papers containing the principal results as they were obtained were published in the *Astronomische Nachrichten.*

When Asaph Hall began to observe with the 26-inch refractor at Washington in 1875 the satellites of *Saturn* were some of the first objects which claimed his attention; and in the years 1875, 1876, and 1877 he made a series of observations of the differences of right ascension and declination of the planet and its most distant satellite, *Iapetus.* The mean distance of the satellite determined from such observations depends mainly upon the differences of right ascension; and as this mean distance is the principal factor in determining the mass of the planet, we thus obtain a calculation of this mass, which is practically free from

---

[*] Beobachtungen der Saturnstrabanten. Erste Abtheilung. Beobachtungen am 15-zölligen Refractor. Supplément I. aux observations de Poulkova. 1888.

[†] Beobachtungen der Saturnstrabanten am 30-zölligen Pulkowaer Refractor. Publications de Poulkova. Série II. tom. xi.

1903MNRAS..63..281T

the error of the micrometer screw. The value of the mass of *Saturn* so obtained was published by Asaph Hall in the *Monthly Notices* for 1883 November. It is considerably greater than Bessel's value,[*] derived from observations of *Titan*, and it was the discrepancy between these two determinations which first directed Hermann Struve's attention to the satellites of *Saturn*. The difference was about 6 parts in 1000, and could only arise from systematic errors in one or both of the series of observations; and it occurred to Hermann Struve that the cause of such errors was probably to be found in the difficulty of connecting the satellites with the planet's disc. He decided, therefore, to depart from the usual method of observations, and, in accordance with a suggestion previously made by his father, to measure the distance of one satellite from another. This meant a considerable addition to the labour of reduction, for in each equation he was liable to twelve unknown quantities instead of the usual six which represent the elements of a single orbit; and anyone who has used the method of least squares knows how rapidly the numerical work increases as we add to the number of unknowns. But our Medallist was not one to shrink from labour if he could secure material improvement; and after satisfying himself by a few preliminary experiments[§] that the method was promising, he set out unhesitatingly on the gigantic task which is now happily accomplished.

He began with the outer and brighter satellites, which were easier to observe. At the two oppositions of 1884–5 and 1885–6 he compared the positions of *Iapetus* and *Titan* on 93 nights, *Titan* and *Rhea* on 72 nights, *Rhea* and *Dione* on 39 nights. Each comparison involved several observations, and gave two equations with 12 unknowns for correcting the elements of the orbit; though, by assuming the orbits of *Rhea* and *Dione* to be sensibly circular, the 12 unknowns were reduced in some cases to 9. The mere statement of these equations involves 25,000 figures, which may give a vague idea of the total amount of work involved in their formation and solution by least squares. The solution was performed twice over—once with all generality and the second time assuming the relation between the mean distances of each pair given by Kepler's Third Law—and the observations of the two oppositions were kept separate. When it was seen from the accordance of the results that there were no outstanding questions to be answered, the solution was repeated, using all the material to the best advantage. Moreover these solutions were not mere pieces of computing, but were watched

---

[*] Bessel's value was $\frac{1}{3501{\cdot}6 \pm 0{\cdot}77}$. Asaph Hall's value in the *Monthly Notices*, vol. xliv. p. 7, is $\frac{1}{3482{\cdot}2}$, differing but slightly from his final value $\frac{1}{3481{\cdot}3 \pm 0{\cdot}54}$. ("The orbit of *Iapetus*," Washington, 1885.)

[†] *Astro. Nachr.* No. 2641–2.

1903MNRAS..63..281T

throughout by the keen eye of a man who knew when there was a theoretical weakness in the separation of two different elements. Struve next determined the perturbations due to the Sun, to the non-spherical shape of *Saturn* and his ring, and to the satellites themselves; and thus finally obtained corrected elements of the orbits of these four satellites, *Iapetus*, *Titan*, *Rhea*, and *Dione*, for the epoch 1885.

He then returned to the perturbations just mentioned. To calculate these we must assume values for the masses of the satellites and of the non-spherical portion of *Saturn*, and since some of the perturbations are cumulative, any error in these assumed values will become manifest with lapse of time. Having now got a set of good elements for 1885, he compared them with old observations from 1787 onwards, and deduced improved values for the secular changes due to the perturbations. Thence it was but a step to determine the mass of *Titan* and the non-spherical portion of *Saturn* with considerable accuracy, the influence of the other satellites compared with these being too small to give good values of their masses. For *Titan* he found the value $\frac{1}{4678}$ of that of *Saturn*. Four years previously Newcomb had deduced the value of $\frac{1}{10000}$ from a beautiful piece of work on the motion of *Hyperion*; and Tisserand had arrived at a similar result by two independent methods. Mr. Ormond Stone, on the other hand, showed cause why both these determinations should be corrected to about $\frac{1}{1200}$,[*] so that there was considerable uncertainty in this element when the work of our Medallist appeared. Almost simultaneously with it, however, G. W. Hill[†] found from *Hyperion* the value $\frac{1}{4714}$, and soon afterwards Mr. Ormond Stone[*] found numerical errors in each of the other determinations which brought them into satisfactory accordance with those of Struve and Hill, so that order was evolved out of chaos in a sudden and remarkable manner.

In this first memoir there is also a determination of the mass of the spheroidal excess with the ring-system; but this was revised in the second memoir and may be passed over for the moment. But there is still one very important result of the first series of observations which calls for notice, namely, the determination of the mass of *Saturn*. The mass of any planet compared with that of the Sun follows immediately from the observation of two things: first, the period in which a satellite of the planet revolves round it; secondly, the mean distance at which it revolves. Now given a sufficient lapse of time there is never any difficulty about the first of these; by watching a large enough number of revolutions, even rough observations at the beginning and end will give an accurate value of a single period. The real difficulty is with the second factor, the mean distance, or, say for simplicity, the greatest distance at which the

---

[*] *Bulletin Astronomique*, tom. v. p. 350.

[†] *Astronomical Journal*, No. 176.

1903MNRAS..63..281T

satellite is seen from its primary. It is humiliating to think that at the present time we cannot measure a distance of 500″ by differential transits within 0″·5, and yet this is actually the case. The values for the mass of *Saturn* found by Bessel and Asaph Hall, quoted at the beginning of this Address, differ by 6 parts in 1000, which corresponds to a difference of 2 parts in 1000 in the distance, or about 1″·0. The work of our Medallist supports Bessel's value on the whole, bringing it rather nearer, however, to that of Asaph Hall; and the latter repeated his observations on *Iapetus* in 1890, with the result of approximating slightly towards Bessel. But the discordance obstinately refuses to be reduced below 0″·5. In his second memoir Struve gives (p. 239) as a final result 1/3495·3; and Asaph Hall's final result is 1/3485·7, leaving half of the original discordance still unexplained. (In 1885–7 Asaph Hall, jun., found the value 1/3501 with the Yale heliometer.) Probably some form of "personal equation" is responsible for the discrepancy. We are familiar, for instance, with errors of transit arising from differences in brightness: if we measured the distance between a bright star and a faint one by observing the interval between their transits, we should not expect to get a correct result without applying a correction for "magnitude-equation," which would be of different sign according as the bright or the faint star came first. If they could alternate in precedence (as a satellite and its primary do), we could get the correct distance from the mean of a pair of observations without knowing or applying the magnitude-equation. For errors of this kind we should naturally be prepared in the case of comparisons, by transit, of a brilliant planet with its satellite; and it is easy to recognise that Asaph Hall's observations at least are affected in this way;[*] for the residuals systematically change with the relative distance of planet and satellite. But this will not explain distances being observed too small whether the satellite is east or west of the planet. For this one or other of two suppositions must be made: the first is that the second transit is always observed too soon, whether it be of *Saturn* or the satellite. In micrometer work we are familiar with such errors; for instance, in his micrometer observations of the inner satellites Struve found that his measures of distance differed systematically from those of his assistant; and following up the matter he measured the relative distances *ab*, *bc*, *cd* of four stars *a*, *b*, *c*, *d* in a line, and compared their sum with the total distance

---

[*] If we take, for instance, the observations in *Astronomical Journal*, No. 253, and group the residuals together for which the difference of transit was greater or less than 30^s, we get

	s	″		s	″
over	+30	$v = -0·24$	over	−30	$v = -0·37$
under	+30	$v = +0·21$	under	−30	$v = +0·18$

The earlier observations in 1875–7 are similarly affected to less degree.

1903MNRAS..63..281T

ad, concluding that his own single measures were too small by 0″·088. Even in differential transits cases of the same kind have been found; for instance, in finding the value of a revolution of the screw of a double-image micrometer the screw is turned so that two images of a star are separated by a given interval which is measured by differential transits; and it is generally found that the deduced screw-value varies with the separation, pointing to some systematic error in measuring distances by transits. The error is only large, however, when the two images are very close, and we should scarcely expect to find a sensible error of this kind in the case of objects separated so widely as *Saturn* and *Iapetus*. A second possible supposition is that the difference in brightness of *Iapetus* at Eastern and Western elongation, which amounts to fully two magnitudes, is responsible for the discrepancy. But in either case we must admit that such errors are also likely to affect the relative distance of *Titan* and *Iapetus*, so that the method of Struve would not be *in this case* freer from suspicion than that of Asaph Hall. For the present, while recognising the great merit of Struve's work, we can merely place his result for the mass of *Saturn* alongside of that of Asaph Hall, awaiting further light on this most difficult puzzle, which will perhaps come from further heliometer measures.

Let us now turn to the second series of investigations. The second memoir is nearly three times as long as the first, and probably represents at least three times as much work. Figures are apt to be wearisome, and I must not give too many; but some figures are necessary in order to convey any idea of the marvellous diligence of our Medallist; while to appreciate it properly is no doubt impossible unless one has laboured in the same field. Most of us can understand, however, that considerable labour is necessary for the computation of a single orbit: Struve makes 14 determinations of the orbit of *Rhea*, 14 of *Tethys*, 7 of *Enceladus*, 6 of *Hyperion*, 5 each of *Mimas* and *Dione*, and 2 of *Titan*—53 orbits in all—and this is of course only a fraction of his work. Before it came all the observations, made with his own eyes; after it came an elaborate discussion of the changes in the orbits, leading to results to be presently mentioned.

The observations are certainly beautiful. Taking the system *Tethys-Rhea*, the probable error of a single observation comes out from solutions in seven successive years ±0″·056, ±0″·059, ±0″·059, ±0″·064, ±0″·069, ±0″·069, and ±0″·066. Any other sets might be substituted for these. It is scarcely conceivable that an observer should have a more uniform habit than this, and it is doubtful whether any of the great visual telescopes of the world has ever been, or ever will be, used to better purpose. There are 1300 of these beautiful observations, and the mere statement of the equations for reducing them, which in the last volume ran to 25,000 figures, now requires over 100,000. The total work must have needed millions. Comment is not only

1903MNRAS..63..281T

difficult but superfluous; and we will pass to the more noteworthy of the results, especially the unforeseen results, of all this eminently skilled labour.

The first has reference to the position of *Saturn's* equator. Before Struve's work this had been taken to coincide approximately with the plane of the orbits of the satellites, and there was no accurate knowledge of any real differences, which are certainly small. But if the orbit plane of any satellite is inclined to the plane of *Saturn's* equator, it follows from the theory of perturbations that the inclination will remain constant, though the plane will revolve with constant angular velocity, assuming that the spheroidal shape of *Saturn* is the chief perturbing cause. Struve found that he could choose the plane of *Saturn's* equator, so that the orbit of *Tethys* was inclined to it at a constant angle of 65′·1, while its plane revolved with a uniform velocity of 72°·8 per year; and the orbit of *Rhea* was inclined to it at an angle of 20′·5, revolving at a rate of 10°·2 per year. In fact he detected the true position of *Saturn's* equator for the first time, and gave a precision to his result which led him to a discovery of a remarkable kind, as we shall presently see. To follow, however, the course of events, he determined the eccentricities and motions of the apsides of *Enceladus* and *Dione*; and not only a large eccentricity, but a considerable inclination for the orbit of *Mimas*.

Now we have seen that Struve calculated the orbits of all these satellites, not only once, but several times; and he was able to detect, therefore, not only the values at a single epoch of all these eccentricities and inclinations, many of which he discovered for the first time, but the changes in them from epoch to epoch. In this way, as has just been mentioned, he found the rate at which the planes of the orbits of *Tethys* and *Rhea* were revolving on the plane of *Saturn's* equator. He also found—

1. That the Perisaturnium of *Enceladus* is revolving at such a rate that the conjunctions of *Enceladus* and *Dione* always occur in that direction. This actual result was new, but a similar result had been already found by Newcomb in the case of *Titan* and *Hyperion*, and shown to follow from the near commensurability of the mean motions.

2. But in the case of *Mimas* and *Tethys* Struve found a law of perturbation which had not been anticipated even by analogy. The conjunctions of these two satellites oscillate about the direction midway between the *ascending nodes* of these orbits on the plane of *Saturn's* equator. This is the remarkable discovery above referred to; for it is needless to point out that it could not have been made unless he had first determined the plane of *Saturn's* equator with great precision. And there were further important consequences. The motion of *Mimas* and *Tethys* had long been a puzzle. Marth had recognised that the mean motion of *Mimas* was variable and had taken refuge in the hypothesis of a secular acceleration, and Dr. Bohlin had found, from the dis-

1903MNRAS..63..281T

cussion of old observations, that there were changes in the mean motion of *Tethys*. The reason of both these anomalies was now explained by the oscillation or libration of the line of conjunctions, which gave, in fact, a long inequality of seventy years' period and considerable amplitude. Since these curious perturbations have coefficients depending on the masses of the tiny satellites, it was possible to find the masses of *Mimas*, *Tethys*, and *Dione*; and upper limits for those of *Enceladus*, *Rhea*, and *Iapetus*; and it appears that the masses found in this way are smaller than would be expected from the brightnesses compared with that of *Titan* in a ratio which becomes more pronounced as we approach the planet—viz. the masses of *Rhea* and *Dione* are about seven times, *Tethys* thirteen times, and *Mimas* as much as twenty-six times, smaller than the brightness would suggest. Either the density of the inner satellites is much less or their albedo much greater than that of the outer, and this is in accordance with what is known of *Jupiter's* system.

We cannot sufficiently admire the skill with which our Medallist proceeds from one discovery to another, making each secure and complete as he goes, so that it may serve as a sure stepping-stone to the next. From his beautiful observations he calculates a series of orbits, which can only be reconciled by rectifying the plane of *Saturn's* equator; thence he proceeds to the changes of the orbits; and though some of these are quite novel in character, he unravels them with the same ease with which he made the observations. Thence he steps to the masses of the tiny satellites; and finally to an approximate law of physical relationship which may ultimately throw light on the remote history of the Saturnian system. All this with a facility and elegance which make it clear that he is a complete master of all his weapons, from the giant 30-inch telescope to the complexities of gravitation analysis.

And I have by no means completed the list of his achievements. He found also the compression of the body of the planet from the perturbations of the satellites, and his result was in good accordance with the compression derived from measures of its shape. He found that the Ring could not have a mass much greater than that of one of the satellites, which affords a fresh confirmation of the correctness of Clerk-Maxwell's views with regard to the constitution of the Ring. If we were tempted for a moment to wonder whether the story so beautifully unfolded by our Medallist belonged to the regions of romance rather than of solid fact, we should be at once reassured by such independent confirmations as these. It has already been remarked how his value for the mass of *Titan*, which at the time of publication seemed far indeed from any previous suggestion, was almost immediately confirmed from three independent sources. His observations of *Mimas* and *Tethys* elucidated the difficulties found by Marth and Bohlin. His series of values for the masses of the satellites finds an analogue in the system of the planet

1903MNRAS..63..281T

Jupiter, and is a new proof of the unity of the solar system. Two totally independent methods of measuring the ellipticity of *Saturn's* disc are mutually confirmatory; and finally his value for the mass of the Ring must be associated with the spectroscopic experiment of Keeler as a beautiful and independent confirmation of the work of Clerk-Maxwell thirty years before.

We have still to notice a case where Struve not only confirmed the work of others, but added to it in a striking manner. It had been shown by Newcomb that the conjunctions of *Hyperion* and *Titan* were linked in perpetuity with the Aposaturnium of *Hyperion*. This discovery was, in fact, the prototype of Struve's discovery of a similar relation for the orbits of *Enceladus* and *Dione*, and has already been referred to. In such cases it does not follow that the linkage is absolute; the conjunctions may librate or swing from side to side of the line of apsides, though they never ultimately depart from it; but Newcomb had supposed that in the case of *Titan* and *Hyperion* there was no sensible libration of this kind. The searching scrutiny of Struve, however, detected a libration of 640 days' period, which may alter the longitude of *Hyperion* by ±9°. Let us consider for a moment what this means in the language of our every-day life. Sailors divide the compass into thirty-two "points," each of which is therefore about eleven degrees. It would be regarded as a slight, though sensible, alteration in the course of a ship to sail her a single "point nearer the wind," and viewed from a distance this change, of course, might be difficult to detect. A thousand million miles away a tiny satellite is sailing in its orbit with what might seem like precise regularity; but the keen vision, optical and mental, of our Medallist detects that every two years it strays about a "point" from its expected position, which he considers a vagary so large that it could not possibly be overlooked! Such is the advance which astronomical science has made in the three centuries which have elapsed since Galileo first saw that there were other satellites besides our own Moon.

In the years 1890–1892 the ring of *Saturn* closed up to a thin line of light, and measures were made of the ball and rings. Eclipses of the satellites were also observed at Pulkowa and elsewhere, and used to determine the dimensions of the ball by an independent method the accuracy of which can be estimated from the fact that the value of the semi-major axis of the planet at mean distance found at Pulkowa was $8''\cdot720$, and at Princeton $8''\cdot716$ (*Monthly Notices*, liv. p. 465). Struve neglects no opportunity, as is well exemplified in his treatment of the numbers of old observations of the satellites. To give one instance: Some transit observations of *Iapetus* and *Saturn* were made at Marseilles in 1787, which were published by Lalande and used by him to determine the orbit. But there was obviously something wrong with them. The original records of the observatory before 1792 had been lost, as Asaph Hall ascertained; and after spending much time in trying to divine some misunderstanding which

1903MNRAS..63..289.

would make sense of them, he gave them up as hopeless. Coming later on the field, Struve might well have been content with the verdict of so able an astronomer as Asaph Hall; but he spared no pains to get at the truth. He selected nine of the best observations for reduction, and from a study of the results concluded that the transits must have been observed across a bar inclined at an angle different from that assumed by Lalande.

By this imperfect sketch I hope to have brought before you in some degree the sterling qualities of the work which we are honouring to-day. I hope to have shown that it is of the first order both in quality and quantity; that our Medallist is not only a skilful observer, and an untiring worker, but in the front rank as a mathematician. For this rare combination of qualities in the man himself, and for what he himself has done alone, he receives this token of our highest esteem.

But it is natural that our thoughts should stray from the present occasion to the past; from the man himself to his father, who was awarded our Medal more than half a century ago; and to *his* father, who received it a quarter of a century before that. For the third time we welcome the name of Struve to our list of honour, and a welcome as cordial awaits an unlimited number of those who in the future may produce work of the same quality as their forefathers. On one fact we dwell with especial pleasure: our hearts warm to think that the venerable figure of Otto Struve has been spared to see the day when his son receives this mark of our esteem. All those who formed our Society when he received our medal in 1850 have passed away; but two of our associates—Otto Struve and Johann Gottfried Galle—remain to tell us that devotion to astronomy enables men to defy the march of years. In a letter dated January 13 Otto Struve regrets that infirmities prevent his undertaking the journey from Karlsruhe to England in order to be present at our meeting, but he begs me to greet the Society cordially from him. Our Medallist himself also finds that he cannot put aside his engagements to be present to-day; and while for our own sake we cannot but regret his absence, we recognise in it another instance of that self-denying devotion to duty without which he could not have carried out so successfully and completely this great work.

The Chairman then, handing the Gold Medal to the Councillor of the German Legation, addressed him in the following terms:—

"Count von Bernstorff, we thank you for being present to-day to receive the Medal on behalf of Dr. Hermann Struve. In transmitting it to him we beg you to assure him that this formal recognition of his work is but the natural outcome of the keen interest and appreciation with which it has been followed by the astronomical world for many years past; and that we are glad to think that its value to him will be enhanced by the knowledge that it ranks him with his venerable father and his famous grandfather."

1903MNRAS..63..289.

Note added 1903 *February* 1.—In the "Report of the Council of the Society to the First Annual General Meeting, 1821 February 9," there is the following paragraph (*Mem. R.A.S.*, vol. i. p. 25) :—

"The subjects of physical astronomy are so various, and of so mixed and complicated a nature, that the Council defer to a future opportunity their observations on this head. In order, however, to show their disposition to encourage the consideration of such subjects (unhappily too much neglected among the geometers of this country) they recommend the proposal of the Society's gold medal and twenty guineas for the solution of the following prize question :—

> "'For the best paper on the theory of the motions and perturbations of the satellites of *Saturn*. The investigation to be so conducted as to take expressly into consideration the influence of the rings, and the figure of the planet as modified by the attraction of the rings, on the motions of the satellites ; to furnish formulæ (adapted to the determination of the elements of their orbits, and the constant coefficients of their periodical and secular equations) from observation ; likewise to point out the observations best adapted to lead to a knowledge of such determination. The papers to be sent to the Society on or before the 1st day of February 1823.'"

Apparently no reply was received before the specified date ; but after the lapse of 80 years the Gold Medal is bestowed for a complete solution of the problem proposed at the foundation of the Society.